

Dr. J. M. (copy)

5.

OBSERVATIONS

ON

P U E R P E R A L F E V E R

AND

P U E R P E R A L M O R T A L I T Y ,

IN REPLY TO DR. EVORY KENNEDY'S PAPER, ENTITLED
"ZYMOTIC DISEASES, AS MORE ESPECIALLY
ILLUSTRATED BY PUERPERAL FEVER,"

MADE AT THE MEETINGS OF THE

D U B L I N O B S T E T R I C A L S O C I E T Y ,

SESSION 1868-69.

BY

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DUBLIN:

JOHN FALCONER, 53, UPPER SACKVILLE-STREET.

PRINTER TO HER MAJESTY'S STATIONERY OFFICE.

1869.

DR. KENNEDY'S PROPOSITIONS.

1. That puerperal metria is due to the absorption of a poison by the parturient female.

2. That this poison may be generated by any parturient female; and, where the circumstances are favourable to its imbibition, it may be absorbed into the system of the generator or that of any other parturient female exposed to its influence.

3. That the generation and absorption of this contagion is in a direct proportion to the number of parturient females cohabiting at their parturient period, or who breathe the same atmosphere when lying-in.

4. That in lying-in hospitals, where large numbers of patients are delivered under the same roof, this disease finds its habitat, appearing and reappearing at uncertain intervals.

5. That its appearance, although apparently capricious, is not infrequently traceable to the occurrence of other zymotic diseases, to a general unhealthy state of the hospital, the labours for some time being succeeded by bad recoveries, before the true zymotic metria exhibits itself.

6. That it is produced by contagion long experience proves, following in the steps of certain practitioners, whilst others are totally free from it, and that in the same locality.

7. It is endemic, confined in its occurrence to certain localities.

8. It is not only confined in its occurrence to a given hospital, but it is observed to haunt certain wards of the hospital, and this to such a degree that I have been obliged to close up for many months wards in which it established its special habitat.

So much for our positive proposition.

Now for the negative; and whilst we freely admit that negative evidence is less valuable, we look upon it that here it is essential to complete the chain of reasoning.

9. Zymotic metria is not a disease peculiar to parturient women confined in their own houses, occurring comparatively rarely among them.

10. It is not a disease observed to occur in small lying-in hospitals or cottages where only one or two patients cohabit in their lying-in.

11. The just and inevitable conclusion from the foregoing propositions is that by continuing the system of large lying-in hospitals we are causing the death by zymotic metria of a number of patients for one that would occur under any system that would secure the separation or isolation of women in their confinements.

12. But as hospitals possess advantages facilitating the cure of patients and as schools of instruction, it is quite possible to combine these advantages with those of the separate system by means of grouped but isolated cottage or pavilion hospitals, with only one, or, at most, two beds in each isolated room.

13. That consequently with our present knowledge of puerperal fever the conclusion is inevitable that the mortality among parturient women would be greatly lessened by an alteration in the construction and arrangement of lying-in hospitals.

OBSERVATIONS

ON

P U E R P E R A L F E V E R

AND

PUERPERAL MORTALITY.



DR. JOHN A. BYRNE said he agreed with those who were of opinion that Dr. E. Kennedy was entitled to the best thanks of the Society for the papers which he had read. Nay, he would go further and say that the whole medical profession of the country and the public at large were indebted to him, as his paper had been the means of inducing a discussion upon a topic of such overwhelming interest as the prevention of metria or puerperal fever.

It is a curious fact that although many valuable contributions to the history, pathology, and etiology of this disease have emanated from some of our distinguished men here (a circumstance not to be wondered at considering the ample opportunities which have been, I may say, almost constantly and unfortunately afforded of studying this disease in this city), yet this is the first time that the matter has been formally brought before the notice of the profession in the manner in which it has now been brought forward—viz., for the purpose of eliciting information as to the best means of preventing its invasions in future. Whether, in fact, a different system of relief to the parturient female is to be provided from that which has been in operation in this city for more than a century, and which had been found to be attended with many and great advantages, certainly to suffering womanhood; and one which, were it not indeed for the occasional occurrence of this formidable malady, would be superior to any other system which could be adopted.

Of such importance does Dr. Barnes, ex-President of the London Obstetrical Society, one whose name is familiar to all obstetricians, deem

^a See opposite page for Dr. Kennedy's Propositions.

the discussion of this subject, that in the year 1865 he thus expressed himself, in making some remarks upon a paper read by Dr. Snow Beck on puerperal fever :—

“The president said that the subject was one of such paramount importance, that it deserved a special discussion every year. He believed that if attention were thus brought continually to it, that much would be done in the way of preventing this, the most destructive of all causes to puerperal women.”—*Obs. Transactions*, Vol. vii. 1866.

In the year 1858 the Imperial Academy of Medicine of France deemed the investigation of this malady, which committed such fearful ravages in its hospitals, of such great import that it devoted several *séances* to its discussion; and when the names of Velpeau, Depaul, Beau, Piorry, Crnveilhier, Bouillaud, Trousseau, Cazeaux, Dubois, Danyau, &c., &c., are mentioned as having taken part in this discussion, and contributed their experiences, it may be inferred that the subject was handled by no pretenders to science, but by men of world-wide reputation as pathologists and obstetricians. The result of their discussions is contained in a volume published in Paris by Bailliere, 1858. I shall have occasion to refer to it again in making some quotations from it which bear upon the present subject.

We must not suppose, however, that the investigation of this disease has been neglected by the earlier or later writers, for we have been inundated with works upon the subject from the time of Strothers, who was the first to term the disease puerperal fever, to the present day. Indeed I do not exaggerate when I say the name of works upon this disease is legion, and no greater proof can be given of the great attention which this subject has received at the hands of medical men than the multiplicity of essays and volumes upon it. It has in fact received more attention, I think, from medical writers than any other affection.

Dr. E. Kennedy's paper may be said to consist of four parts or divisions, viz.:—In the first part or introduction he discusses the probability of all zymotic poisons being ascribable or referable to one common principle, modified by a variation in natural or physical circumstances or conditions.

In the second part he treats of the disease, or rather the various forms of the disease, and details many cases illustrative of the different forms of metria, such as had occurred in his experience, and his treatment of them.

In the third part he lays down his thirteen propositions, from which he makes his deductions.

And in the fourth he proposes his remedies.

Now with regard to the first supposition, for in the present state of our knowledge of this subject we cannot call it by any other name, although there are many facts and circumstances which may lead us to the belief

that those zymotic diseases may have one common origin, and that circumstances may modify the one poison in such a manner as to cause so many different developments of it, and although it is a matter of notoriety that at a time when metria is epidemic other zymotic diseases are very often prevalent, yet we have no marked data sufficient to warrant us in coming to the conclusion that there is any one single zymotic influence, and that all those diseases spring from or are developed from the same germ. Hence it is better in the present state of our knowledge of this obscure and difficult subject not to be positive, nor draw deductions which are determinate and which are not warranted by facts. Indeed my friend Dr. Kennedy, I must say, is not positive upon this matter, but is content with saying that should the principle of *isomerism* be adopted the difficulties in accepting the idea of a common morbid poison would be lessened.

The second part of his paper, or, to speak in more correct language, the second portion or division of the first part is devoted to practical suggestions, and the diagnosis of metria or puerperal fever from its congeners.

Dr. E. Kennedy recommends the use of the lancet in ordinary metritis or metroperitonitis, and states that those are the cases which bear depletion best, and that the use of the lancet is too much neglected in them. He says that twelve to eighteen ounces of blood extracted from the arm, followed by eighteen to twenty-four leeches, will often relieve and subdue all inflammatory symptoms, and the pulse that was hard, contracted, and compressed will, as in ordinary peritonitis, rise to a more rounded resistance. He says in the puerperal fever of the present day the use of the lancet is rarely admissible, local depletion must be our sheet anchor, but in having recourse to it the secret of success is to reduce the pain by repetition of leeching before reaction has had time to establish itself, &c., &c.

With regard to venesection I differ *toto cælo* with Dr. Kennedy, and in no instance would I attempt or allow the use of the lancet. As well might you bleed in typhus on account of the headache as in this disease, on account of the abdominal pain, which is but the local manifestation of general poisoning of the whole system. Venesection has been recommended by men of great reputation—Denman, Hay, Clarke, Dewes, and many others; but, alas! I have not the least doubt in my mind that many were killed by this treatment, and that perhaps the great mortality which has attended this disease may be accounted for by the antiphlogistic plans of treatment recommended by our predecessors (who loved the lancet not wisely, but too well), and who, had they been content with trusting more to nature and less to physic, might have had more success in their practice. The same may be said of leeching, which must be practised with extreme caution. No doubt in cases of pure metritis, or peritonitis,

or metropéritonitis, leeches cautiously applied over the affected regions are useful; but where puerperal fever or metria prevails, I would not follow Dr. Kennedy's advice to leech and leech again *coup sur coup* until pain is abated or altogether removed. I would much prefer to trust in opium, poultices, removal of the patient to a purer air, and watching the moment to exhibit brandy, wine, quinine, thus endeavour to prevent the tendency to death which is by exhaustion. Those who have witnessed real puerperal cases will call to mind the rapidity with which tympany, diarrhea, effusion into the bronchial tubes, and collapse set in; and no person now-a-days would have the temerity to order from sixty to one hundred leeches to be applied to the abdomen of an unfortunate woman whose whole system is poisoned by this metrial poison, whatever it may be. Yet such was the practice recommended by Campbell, who, not content with one hundred leeches, afterwards encouraged the bleeding by stupes, poultices, &c. Dr. Collins recommends the use of three or four dozen leeches, followed by the warm bath. Can we wonder for a moment that such a fearful mortality attended the disease? Is it not in fact a matter of surprise that any poor victim ever recovered from such treatment? I have read in works upon this subject cases in which I have not the slightest doubt in my mind that the patients were—I will not say killed,—but, to use the most charitable phraseology, they were hurried to the grave.

The same may be said of calomel, and other preparations of mercury. In simple peritonitis or metritis it may be admissible, but we must be very guarded in its exhibition, even in those cases. Nature never sets up an inflammation of this kind without some object. By our interfering with the efforts of nature we may do premature mischief in even mild cases. But what can I say of the treatment recommended by Dr. Kennedy at pages 274, 275 of his paper. He says, "When metria or peritonitis were to be anticipated, as when the disease was prevalent, after manual interference in labour, or for the removal of the placenta, my habit was to commence at once after the labour was completed with small and frequently repeated doses of mercury—a grain or grain and a half of grey powder every third or fourth hour, and even, in more suspicious cases, applying mercurial ointment to the armpits and groins. By those means, without irritating the patient's system to any serious extent in the course of forty-eight or fifty-six hours, a slight mercurialization may be perceptible, often before the time at which the disease could grapple the patient." And then he goes on to say:—"Should metria show itself by pressing mercurials a little more freely the system may be speedily brought under its specific influence, and the disease be thus speedily or easily checked, if it had not been prevented. I have only," he says, "on one or two occasions seen puerperal fever

prove fatal after mereurialization showed itself, and in them the action was complete. But, on the other hand, how often have I seen mercury fail in every dose in producing the slightest approach to its specific effects." He then mentions one case, where by accident it did produce its specific effects, and the woman, already mercurialized, was attacked by metria, and she died on the third day.

This line of practice, I must confess, appears to me to be most erroneous, and, if adopted generally, is calculated to lead to most injurious results. What would be thought of the surgeon on the battle field who would commence to plunge his knife into the most vital parts to probe for balls which had never penetrated, or who would amputate limbs for injuries never received? Would he not be deemed insane, and be quickly removed from his post? Yet exactly similar does it appear to me is the practice laid down for our guidance by Dr. Kennedy. He recommends you to treat the disease before it comes; to saturate your patient with mercury; to reduce, in my opinion to nil, the chance which the poor patient has of recovery, should she be unfortunately attacked.

It is a sad thing to reflect, sir, that in such an advanced age as this is, such doctrines should be taught, and that, too, by so distinguished a member of the profession as the ex-master of the Rotunda Lying-in Hospital, one whose experiences extend over a period of ten years as assistant and master, and under whose observation so many thousands of cases had passed. Nay, as Dr. Kennedy stated a few evenings since, he had been thinking on this disease for forty years; so that the opinions and doctrines laid down by him are not those of a young and raw enthusiast, but of a philosopher of forty years' standing.

I shall now, sir, come to the really important part of the paper, which was commenced on the second evening of the discussion, and I have only to regret that Dr. Kennedy should, in the latter part of his first evening's paper, have descended from his chemico-pathological Pegasus, and condescended to enter into the details which occupied a considerable portion of the evening.

The second part of the paper commences with the statement, that the whole family of puerperal diseases—puerperal fever, metritis, peritonitis, pleuritis, phlebitis, arthritis, pyemia, purpuric or cerebro-spinal metria, traumatic metria, erysipelas, and hospital gangrene—may be all classified under the head of metria. To this statement, in my opinion, no objection can be possibly entertained, as although in different epidemics different pathological lesions had been observed, yet still, for the sake of simplicity, and for the reason that we know so little about it, or *faûte de mieux*, as the French say, we shall accept this proposition, and agree with Dr. Kennedy, that metria is to be assumed as representing all the different manifestations or developments of the same poison or zymosis.

On this broad foundation of zymotic metria he has raised up the thirteen now celebrated superstructures or propositions, which, being in print, must be familiar to the Society.

No. 1. There is no rejecting this proposition, in my opinion, and I believe it will meet with universal acceptance.

No. 2. This proposition is difficult to understand in many respects, but it means exactly this, if I interpret Dr. Kennedy correctly, viz., that any parturient female may generate a poison, then absorb or imbibe it herself, or those in her neighbourhood may be infected from this poison generated by her.

Now this theory of the origin of the puerperal poison and its elaboration, which Dr. Semmelivius was the first, I believe, to propound, and which Dr. Kennedy assumes as a matter of certainty, is in fact the keystone of the arch of this proposition, and is one the perfect truth of which should be established before it is given out *ex cathedra*, as it is in this proposition. Up to the present time, I think (and I speak under correction), this explanation only merits the name of a very ingenious theory to define the mysterious origin and mode of propagation of zymosis.

If this explanation of the origin of metria be the true one, it would differ in some respects from other zymotic poisons. Take scarlatina, for instance. Will anybody assert that it is produced in this manner? Does a person in a healthy condition become the generator firstly of scarlatina and then the imbibitor of it? If so, this is a most remarkable illustration of *le cercle vicieux*. Now, I think that scarlatina, as an example, may be observed as making its appearance in two different ways:—Firstly, from some cause the poison is developed in the whole system, not in an organ, or, secondly, the person being exposed to the contagion is attacked, and the disease is thus developed. Are we, I say again, to suppose that the generator of a disease becomes the absorber of the same disease. The notion is a very unphilosophical one, and the fact should be well established before it is made a universal proposition.

Erysipelas may be taken as another, perhaps a more suitable, illustration of this poisoning, inasmuch as the position of the parturient female has been compared by Sir J. Simpson and others to that of a patient after undergoing amputation or some serious operation: an analogy rather overstrained, in my opinion. However, *ad rem*. A patient is in hospital for some wound or accident; there is not a case of erysipelas in the ward; suddenly, from some cause, the patient is attacked by shiverings, erysipelas makes its appearance; and Dr. Kennedy would explain this phenomenon by saying that the patient generated a poison, and afterwards absorbed it. Now is not a more probable explanation of it this, that pyemia took place from the zymotic

condition of the air of the hospital, and that the blood thus poisoned exhibited those manifestations. The laws of zymosis are so little understood, however, that up to the present we cannot, as I have said, acknowledge that we are certain of the precise means by which blood poisons are developed, and it would be like building a house upon a foundation of sand to found a theory of such importance upon a mere hypothesis.

No. 3. Not agreeing exactly in the view which Dr. Kennedy entertains as to the mode of generation of metria, and which he seeks to impress upon the Society, and also because I think that the very statistics upon which he relies are opposed to his ideas, I must withhold my consent to this proposition.

If we take the statistics of the Rotunda Hospital, and divide the years into groups or series, we find that the reverse of this proposition is true, for it is a fact that the mortality was less when a large number were admitted into the hospital than in similar groups of years when the number of patients was less. The very group of years upon which Dr. Kennedy relies so strongly, viz., the last fifteen years, shows the highest mortality, whereas those groups of years in which the greatest number of females were admitted shows the smallest death-rate. This part of the question has been so recently and so satisfactorily made evident by Dr. McClinton in his able and argumentative observations, that it would be a waste of time to repeat now what he has placed in such a clear light before us; but I may be permitted to say, that the statistics which I have copied from another source, viz., the mortality from puerperal fever occurring in six of the French hospitals, and to which I shall again refer, amply corroborate this important fact, viz., that the death-rate does not seem to bear a direct ratio to the numbers admitted.

No. 4. This proposition, in my opinion, involves a paradox, for it affirms that where a large number of patients are delivered under the same roof the disease finds its habitat, appearing and reappearing at uncertain intervals.

By the habitat of a disease we mean, in fact, the place of dwelling of it; and we must believe, if this hypothesis is true, that metria is constantly in the wards of a lying-in hospital. If it be, therefore, constantly present, or lurking in the atmosphere, how do we reconcile this with the well-known fact, that the disease only makes its appearance now and then, appears and reappears at uncertain intervals, as Dr. Kennedy states.

If this supposition were true, every patient entering a lying-in hospital would be almost certain to be attacked. But is such the case? No. Intervals longer or shorter generally elapse between the occurrence of outbreaks of this disease, and sometimes it will remain in abeyance

for a very long period. Dr. Kennedy has not framed this proposition in very logical language, and I think that it scarcely requires me to say that the proposition contains its own refutation, as a disease to have its habitat, must be always present, therefore it should never disappear.

No. 5. The language in which this proposition is couched is also vague and unphilosophical, for he says that its appearance, although apparently capricious, is not unfrequently traceable to the occurrence of other zymotic diseases, to a general unhealthy state of the hospital, &c., &c.

Now here, in my opinion, Dr. Kennedy is completely at variance with his previous statements. He asserts, in his first and second propositions, that metria is a parturient poison. This I believe myself to be true; but now he asserts that it is traceable to the occurrence of other zymotic disease. He does not specify what, but he in this proposition destroys or makes away with the individuality of metria. If such be not his opinion, his words to my mind convey that meaning, and I think that most of those who have carefully read his paper will agree with me in the interpretation which I put upon his language.

What, may I ask, is the meaning of the word *traceable*? It means to be traced to. Traceable, then, to what? Is it to typhus, to scarlatina, to variola, to erysipelas, or any other zymotic disease? Here Dr. Kennedy leaves us to our own imaginings. He throws no light upon this subject, so full of mystery, but is content with uttering a platitude, which, in my opinion, has neither meaning nor truth, and which does not in the least strengthen his position. He, in the first instance, endeavours to prove the individuality or essentiality of metria, in which, I think, he is correct; and he then, in this proposition, insinuates that it is traceable to, or has its origin in some other zymotic disease. This, I must say, appears to me to be a most glaring inconsistency, and one proposition negatives the other.

No. 6. This proposition has long been the battle ground of two great parties, and arguments have been adduced for and against the contagiousness of metria, which would be a work of supererogation to repeat. Many of those cases have, in the course of this discussion, been mentioned, upon which the contagionists most strongly rely, and certainly many of them are very remarkable, and are such as to induce us to have a strong faith in the doctrine of contagion. On the contrary strong arguments have been given by other writers, which, to a great extent shake the doctrine of contagion. I will not enter here into them; they are amply discussed in almost every treatise upon puerperal fever, and are entitled to the most searching inquiries. As regards my own experience of this malady, I had opportunities, unfortunately—like all those who have made obstetrics their special study, and who have had large hospital experience—of seeing this source in all its forms and types, in

all its degrees of severity, and I must candidly confess that although I do not disbelieve in the doctrine of contagion, yet that I think contagion is not the strongest element in continuing this disease in hospital. I have seen the disease attack perhaps one, or two, or perhaps more, out of several patients in the same ward, all of whom were healthy. Then it would make its appearance in another part of the house. To be sure, every precaution was taken, and the patients were removed to separate wards when the disease had assumed, as it always did, I may say, in the true metria, a bad form. But still I think that if this disease were so highly contagious as it is stated to be, that ample time to propagate itself had been, in many of those cases, allowed before removal. Now, let it be distinctly understood that for one moment I do not class myself amongst the non-contagionists, but that, on the contrary, I have insisted, and will always insist, on every precaution being adopted to stop this scourge so fatal to lying-in women.

With regard to this highly important question of the contagiousness of this disease, and its infectiousness, all the French physicians agreed about the infectious nature of it, whilst there was some difference of opinion about its contagiousness.

MM. Danyau and Depaul are of opinion that the true puerperal fever is of a highly contagious nature, and is not only transmissible or communicable from one patient to another, but that it is communicable by another person as the medical attendant or nurse. M. Depaul goes further, and thinks that, under certain conditions, it may be communicated to non-pregnant women; and he gives the subjoined very remarkable cases, in proof of his statement.

He says that, in the year 1839, a very severe epidemic of puerperal fever raged at the maternity, whilst under his management. On one evening, one of the female pupils, Mademoiselle D., whilst in the act of washing the genital organs of a patient, suffering from a severe puerperal fever, suddenly experienced a most painful sensation, which she referred to the emanations which she had respired on raising the bed covering, and she declared that she felt very ill. On that very evening she was attacked by rigors. The belly became very painful, the pulse small and frequent. Soon after, vomiting and diarrhea set in. The vomited matters were green, and all the other symptoms of puerperal fever were well marked. She died on the third day. On making a *post mortem* examination, all the appearances of puerperal peritonitis were manifest. He adds that this young woman was not in any condition similar to that of a parturient woman, she being a virgin, and not at a menstrual epoch.

He also adds that there was a tradition of similar cases which had occurred at the maternity on other occasions. And he likewise refers to

12 *Observations on Puerperal Fever and Puerperal Mortality.*

two cases cited by M. Tarnier—two sage-femmes of the maternity—who, in 1856, during the prevalence of the epidemic, were similarly attacked, and exhibited all the phenomena of puerperal fever. One of them died and the other recovered. The epidemic at the time was of such severity that it was necessary to close the maternity. In both of those cases the women were at the menstrual period.

M. Trousseau, speaking on the subject, thus expresses himself, quoting an essay by M. Lorain, entitled “On Puerperal Fever in the Female, in the Fetus, and in the Newly-born Infant.” I wish that he had added amongst the wounded of both sexes residing in the proximity of lying-in hospitals; and he goes on to demonstrate the evil effects of this poisonous influence extending from those establishments to the medical and surgical wards of neighbouring hospitals.

M. Dubois is rather disposed to question the contagiousness of it, or rather the facts adduced in evidence of contagiousness by English physicians.

M. Guerard rather inclines to the opinion of M. Dubois.

M. H. De Chegoïn does not believe in direct contagion, because he cannot think that the quantity of miasm carried in this way by a physician to a patient is sufficient to produce this disease.

M. Cazeaux and M. Legroux believe in the contagious nature of the disease.

We thus perceive that amongst those celebrated names there is a great difference of opinion as to the contagiousness of this terrible malady.

No. 7. This proposition, if allowed in its fullest sense, is very subversive—that is to say, if you accept it as meaning that it is peculiar to certain places, or, as Dr. Kennedy says, confined to certain localities in its occurrence. If you allow this, the lying-in hospitals would never be free from it. But we know, and statistics prove it, that all lying-in hospitals are at intervals free from this plague, and that the general rule is, that it makes periodic invasions, sometimes mild, sometimes of such horrible severity, that hospitals are obliged to be shut, and the whole interior deodorized, fumigated, &c., &c., as has happened in the Rotunda, Coombe, Vienna, and French Hospitals. Now, if this disease were endemic, as I have said before, in the strict sense or acceptation of the term, metria would be the rule, its absence the exception, in lying-in hospitals. Such is not the case, but the reverse is the case. Wherefore, I think, we can scarcely give in our adhesion to this very dangerous and insidious proposition.

No. 8. I certainly think that I have observed certain wards to be more affected by it than others. This circumstance, however, may be explained to some extent. Some wards are more healthy, better ventilated, have better nurses, and other circumstances may also modify the amount of disease in different wards of a lying-in hospital.

What, however, does Dr. Kennedy mean by the first part of this proposition, where he says that not only is it confined in its occurrence to a given hospital: does he imply by this that certain hospitals are never free from this scourge whilst others pass unscathed? Does he mean to select a particular hospital or hospitals? I think, sir, that the history of this disease proves that *any* hospital where parturient women are assembled may be attacked by it. No doubt some institutions have had more misfortunes in this respect than others; but I hold in my hand tables of the results of labours in six of the Parisian hospitals, under the care of the most eminent men, and in every one of those institutions metria made its appearance during the same year, corroborating in this manner a circumstance which has long been observed, that when the disease has made its appearance in a large city almost every lying-in hospital is simultaneously (I qualify the expression) attacked.

Can this proposition, worded in so decided a manner, and conveying the meaning that one institution is, *par excellence*, the seat of metria, then be accepted as true? I think not. No doubt there may be, and must be, a cause why puerperal fever most generally selects a place where there is a large assembly of parturient women; but it must also be remembered that the disease commits occasionally ravages external to an hospital, and in places remote from one.

No. 9. With this proposition I think we must agree, if not altogether at least partially, as although metria is not absolutely impossible of occurrence in patient's houses, yet we all must admit that experience proves that it is of much rarer occurrence in them than in the large lying-in hospitals.

It is on this proposition and the succeeding ones—10, 11, 12, 13—that the battle is to be fought between those who advocate large hospitals and those who advocate small hospitals, or the hospitals built on the system of isolation.

No. 10. If we agree with this proposition, we at once concede everything, for it contains the very essence or kernel of the argument. He says it is not a disease observed to occur in small lying-in hospitals, or cottages, where only one or two patients cohabit in their lying-in.

Now, sir, Dr. Kennedy's own paper contains the refutation of this statement, for at page 295 he gives the mortality of

New Ross as 1 in 185,
Waterford as 1 in 295,
Limerick as 1 in 367.

Certainly, in the Waterford Hospital the mortality from metria is small, *i.e.*, in the second group of cases. Dr. Elliot states that out of 2,656 women

delivered since October, 1844, only two have died from metria, or 1 in 1,328. But previously he states that in the first hospital opened there was a mortality from puerperal fever of 1 in 251. Now, if Dr. Kennedy had qualified this proposition by stating that this disease did not occur so frequently or in such a severe manner, I think no gentleman here would disagree with him; but he has asserted absolutely that it does not occur in small lying-in hospitals or in cottages, and he has given the best refutation possible, to his own statement, *i.e.*, by quoting the statistics upon which he relies for victory.

I will now, sir, with your permission, read a letter from my friend Dr. Griffin, who presides over the Killarney Lying-in Hospital:—

“KILLARNEY, *May 13, 1869.*

“MY DEAR DR. BYRNE,—I only got your letter to-day, so I fear my statistics will hardly reach you in time. I have read Dr. Kennedy’s paper in the *Journal*. I think our hospital tells favourably for institutions of the kind, as during the time it has existed no case of puerperal has occurred, notwithstanding the number of bad cases admitted far exceed the average of the same number indiscriminately chosen from the labours occurring at patient’s homes.

“I have seen some dozen cases of puerperal fever during the same period amongst out patients in a district whose average of births in the year would be 120.

“I am sorry I have not more time to give more accurate information, and remain, very truly yours,

“L. T. GRIFFIN.

“ST. ANN’S LYING-IN INSTITUTION, KILLARNEY, OPENED JANUARY 1, 1866.
Number of patients confined up to present date, *viz.*, May 13, 1869, 139

Number of maternal deaths,	.	.	.	.	.	3
----------------------------	---	---	---	---	---	---

Hemorrhage in placenta previa, patient came in exhausted,	.	1
---	---	---

Rupture of uterus—elderly woman—(three previous bad confinements),	.	.	.	.	.	1
--	---	---	---	---	---	---

Peritonitis after prolonged labour before admission,	.	.	1
--	---	---	---

3

The last case lived fourteen days after delivery by craniotomy, and had no well marked symptoms of puerperal fever. Other patients were in hospital at the time, and took no disease from her, though attended by the same midwife and ward-maid.

“I have to observe, that of those who enter the hospital many have

had previous bad confinements, and that the poor women in this neighbourhood are not very willing to leave their own homes to be confined, except when urged by fears arising from past experiences. Of the 136 cases of recovery of the mothers, 5 were delivered with forceps, 3 by version, 3 craniotomy, several were complicated by hemorrhage, retained placenta, breech presentation, face presentation, &c. The hospital is well situated on an eminence, and consists of a basement, middle, and upper story; the latter comprises two wards, containing nine beds, 15 feet by 13; large windows at each end of each ward, perforated zinc panes; a small ward between, with labour couch; well ventilated corridor. We have seldom more than four patients in together; once there were nine.

“I think the circumstances of the house nearly realize Dr. Kennedy’s suggestion of cottage hospitals.”

We see by this candid statement, that out of 139 cases there was not a single instance of a death which could be attributed to metria. The total mortality from puerperal accidents and effects was 3, or 1 in $46\frac{1}{3}$. Now this case strikingly illustrates the importance of distinguishing deaths from metria from deaths occurring from other causes in lying-in hospitals. Suppose, for instance, that the death-rate of the Killarney Lying-in Hospital appeared in a statistical return as 1 in 46, it would appear a very large death-rate, and we would feel very much inclined to look upon this hospital as one in which metria had its habitat; but what is really the case—

- 1 Died from hemorrhage,
- 1 ,, rupture of uterus,
- 1 ,, peritonitis (simple).

There has not a single case of metria occurred since its foundation. I am sure Dr. Kennedy will feel gratified by my adding this instance to his catalogue of small or cottage hospitals, and to their freedom from metria.

Those, too, who look upon metria as an independent disease, epidemic in its nature, and not depending absolutely upon overcrowding in an hospital, will feel their position strengthened by the reference which Dr. Griffin has made to the outbreak of metria in a district away from the hospital, and which was of such severity that 12 out of 120 deliveries, or 1 in 10, died in their own homes. There cannot be a stronger proof or illustration of the occasional outbreak of the disease than this circumstance, and of its independent origin. It would be very difficult to conceive that those cases originated from a contagious source, as there was not a single case of metria in the hospital; or, if the disease did spread through a contagious influence, the contagion must have been carried in another way, either by some of the attendants or persons in communication with the

parties first affected, for certainly the hospital which had not a single case could not have been the nucleus of this poisonous influence.

I shall now, sir, with your permission, give two more illustrations of freedom from metria in hospitals which have not been alluded to by any previous speaker. My friend Dr. Tate, resident medical officer of the North Union Workhouse, with the sanction of Dr. Minchin, who has charge of the maternity department, has addressed to me the following letter, by which it appears that from the 31st March, 1857, to May 11, 1869, that is, for twelve years, 1,067 cases have been delivered in the maternity department:—

“DEAR DR. BYRNE,—I should have sent this, as you desired, last night, only I thought it desirable to go through the books carefully. I only got them late in the evening. Numbers of these women had twins, and some who are marked married have marks of interrogation after them where Dr. M. thought otherwise, still I put them down, giving them the benefit of the doubt.

“Yours very truly,

“DAVID D. TATE.”

Number of Women confined in the North Dublin Union Workhouse.

From 31st March,	1857, to 31st December, 1857,	83
„ 31st December,	1857, to 31st December, 1858,	78
„ „	1858, „ „	1859, 86
„ „	1859, „ „	1860, 72
„ „	1860, „ „	1861, 93
„ „	1861, „ „	1862, 128
„ „	1862, „ „	1863, 84
„ „	1863, „ „	1864, 97
„ „	1864, „ „	1865, 67
„ „	1865, „ „	1866, 108
„ „	1866, „ „	1867, 72
„ „	1867, „ „	1868, 74
„ „	1868, to 11th May,	1869, 25
Total, . . .		1,067

“Of these 570 were unmarried.

“One, unmarried, got peritonitis on third day by fright from some friend, and died; 2 more from peritoneal fever, 1 from convulsions, and 1 from suffocative catarrh was dying on admission.

“Management of cases under the supervision of Dr. Minchin.

“For about seven years there was a large ward and adjoining closet set apart for lying-in women; now there are two wards higher up.

“No epidemic occurred during twelve years.”

From this table it will be seen that the mortality

From metria was only 1 in $355\frac{2}{3}$

From other causes, 1 in $533\frac{1}{3}$

Total, 1 in $213\frac{2}{3}$

Now, sir, if we compare this with the death-rate which is marked in the tables (page 293) of the Rotunda Hospital, there is a striking difference; and, by a curious coincidence, those twelve years are included amongst the fifteen years which have been selected by Dr. Kennedy to illustrate his point. In what way, sir, are we to account for this striking dissimilarity in results? I believe that an explanation may be offered which will be a solution. The patients who came into the Rotunda to be confined were chiefly citizens, many of them primiparæ, many of them unmarried. However, the workhouse maternity carries off the prize in this respect, for I find that out of 1,067, 570 were unmarried, or more than one-half. And Dr. Tate assures me that this estimate is even under the mark, as the benefit of the doubt was given in a great many cases. But the women who were confined in the workhouse were chiefly those in the prime of health, coming from the outlying districts. Upon this point I speak with reserve, as I have not accurate information upon it, but such was my experience of that institution for a short time that I had charge of it; whereas, those coming to the Rotunda many were already in a delicate state, enfeebled from disease and poverty, and in many cases were already tainted or infected with disease, as Dr. Denham has already pointed out in his remarks at a previous seance, or, at all events, they were predisposed to it.

The next maternity which I shall bring under your notice is that of the South Union Workhouse, which is under the charge of Dr. Jennings. On account of an accident I could not find out the number of births for the same period as for the other workhouse, but during the three years, viz., January 1, 1866, to March, 1869, the number of deliveries amounted to 396. The number of those unmarried bore also a very large ratio to the whole. The total deaths amounted to 5, or 1 in $79\frac{1}{3}$. I could not ascertain exactly how many of those were from metria. My friend Dr. Burne took the trouble of examining the books, and the only one whose death could be positively attributed to metria was one; the others were the victims of disease and debility. The nurse of the wards, a very intelligent woman, assured me that the only occasion upon which there was anything like a dash or outbreak of puerperal

was about twelve years since, when a patient suffering from fatal typhus was brought in to be confined, and several cases occurred very soon after. In this maternity there are two labour wards; one contains six beds, the other five. The rooms are separate; they are on the ground floor, very clean, and form part of the large building on the right hand side of the large yard. The ventilation appears excellent, and a very fine orchard is situate behind the building in which the maternity is placed. There is one particular bed used as a couch, the patients are then transferred to the beds in the other rooms. The rooms are occasionally overcrowded, occasionally empty.

I now come to deal with the first annual report of the maternity attached to Sir P. Dun's Hospital for the year ending March, 1869. This maternity gives attendance at their own houses to females within a circumscribed radius. It is under the medical surveillance of my friend Professor Sinclair, and I need scarcely say that no more accomplished nor able obstetrician could have been appointed to the office.

The number of cases which have been attended externally is considerable, taking into account the short time in which the system has been in operation, viz., 336 from April, 1868, to March 31, 1869; of these only two women died, as the report says, not from parturient causes, or thus—

From metria,	0 in 336,
Other causes,	1 in 168.

This report certainly is only for a very limited period, less than a year, and, of course, it can be scarcely taken as an index of the constant death-rate from out-door attendance. Although the cause of death in the two cases is not mentioned, yet we can safely trust the correctness of the report. It bears evident testimony to the handiwork of that

. "Light Divine,
That deigns on dark '*obstetricy*' to shine."

I allude, of course, to the Rev. Dr. Haughton, who takes so great an interest in medical matters, and whose talents and exertions have done so much to stimulate and promote the science of medicine. As an instance of the beneficial results of out-door attendance it is very satisfactory, particularly when we remember that the gentleman who has the medical charge of the maternity is not one likely to conceal any defects, nor under-estimate any of the disadvantages attendant upon this system, he being the author of that remarkable and trenchant paper or essay, entitled, "Extern Maternities and Lying-in Hospitals, &c. &c. Reply to Article published in the *Dublin Quarterly Journal of Medical Science, &c.*"

I shall now, with the permission of the Society, make a few observations.

Before doing so, however, may I be permitted to direct the attention of the gentlemen present to some points, which illustrate the peculiar mode of reasoning adopted by Dr. Kennedy, and which, to the best of my recollection, have not been noticed by previous speakers.

At page 305, he says:—"Let us further bear in mind that the increased mortality bears nearly a direct proportion to the increased numbers inhabiting each building."

Now, sir, I do not think that it requires any person to be a very abstruse or accomplished mathematician to perceive the truth of this statement. If, for instance, out of 100 patients 2 die annually, we should expect that out of 200 patients 4 will die annually, and so on in like proportion. Is it possible, I may ask, sir, to expect a different state of things?

Can we hope that the death-rate will *diminish* in proportion to the *increase* of numbers, thus, for instance, supposing that there be 1,000 patients, the mortality, according to this rate, should be 20. Pray, would Dr. Kennedy have us expect that only 5, or even less, should die?

Having enunciated this self-evident proposition, that the increased mortality bears nearly a direct proportion to the increased numbers inhabiting each building, Dr. Kennedy proceeds to draw, as it appears to me, the most extraordinary inferences or deductions, or, as he terms them, comparative results, for which I do not think we can find sufficient data in his paper.

He assumes the mean of the mortality of the three hospitals—New Ross, Waterford, and Limerick—as the standard by which to judge the mortality of all lying-in hospitals, this being 1 in 282. Now to this. I think, a strong objection may, *in limine*, be urged, which is this:—There is a considerable variation in the mortality of the three institutions, for whereas the mortality of New Ross is 1 in 185, the mortality in Waterford is 1 in 295, and that of Limerick is only 1 in 367. This for three small hospitals, I think, will be acknowledged to be a considerable variation. But, why has Dr. Kennedy not contented himself with selecting some *one* of these hospitals as the standard, instead of giving us the mean rate of mortality, deduced from the comparison of all three together? Why not, for instance, select the Limerick Hospital, whose mortality is 1 in 367, and say this is my standard, and any hospital whose death-rate is in a greater proportion than this is one which should be closed.

Or again, why should not the Waterford Lying-in Hospital, whose mortality is 1 in 295, be selected as the standard, and every lying-in hospital be expected to show exactly similar results?

Or why not select the New Ross Hospital, whose mortality is 1 in 185, and say, This is my standard; every lying-in hospital must have a similar death-rate.

But, on the other hand, let us suppose that a maternity can be shown whose death-rate differs from those exhibited by Dr. Kennedy, why should not this death-rate be selected as the standard. Let us take, for instance, the Killarney Lying-in Hospital. According to Dr. Griffin's account the mortality in this maternity is 1 in 46·3, although no death occurred in it from puerperal fever.

Dr. Kennedy, however, does not select any single hospital, but having collected a certain number, viz., *three*—and those the three mentioned in his paper—he strikes the average or mean of death-rates in all three hospitals, and he gives that mean as the standard by which to regulate the death-rate of all the hospitals in Europe.

Now, in order to show this point in a clearer light, and that this standard is not to be assumed as the proper one, let us take the mean mortality of the three lying-in hospitals mentioned by Dr. Kennedy, which is 1 in 282; and adding to this the mortality of the Killarney Hospital, which is 1 in 46, see what is the result? why that the standard death-rate is raised from 1 in 282, or ·354 per cent., to 1 in 223, or ·448 per cent., being an increase of ·26 per cent., or over $\frac{1}{4}$. Now, why should not this be taken as the standard instead of 1 in 282?

On the assumption, then, that the death-rate in the three hospitals adduced by him should be regarded as the proper standard, Dr. Kennedy proceeds to draw, as I have said, the most extraordinary inferences, or, as he terms them, comparative results, of which I shall quote only three examples.

No. 1. He says that in the Liverpool Hospital 2 out of 3 die who should not die.

No. 5. In the Dublin, 8 out of 9 die who should not die, &c.

No. 9. In the Paris General Hospital 17 out of 18 die who should not die.

Now, by what process of deduction does Dr. Kennedy infer that those 2 should not die in the Liverpool Hospital, that those 8 should not die in the Dublin Lying-in Hospital, or those 17 in the Paris General Hospitals?

I must confess, that after the most careful and repeated perusals of his paper I cannot find any premises from which those conclusions, so vital, so revolutionary, can be elaborated, except, indeed, the arbitrary standard suggested by the combined results of the three lying-in hospitals *especially* selected by him to the omission of others whose death-rate would have quickly raised this low standard of mortality to a level nearer to that of similar institutions.

It must not be forgotten that this is the cardinal point of the whole discussion, and that its certainty is assumed by Dr. Kennedy in order to establish his position.

What is this method of reasoning but the taking for granted the

conclusion, which at the commencement of the discussion he undertook to demonstrate. This proceeding has been always looked upon as one of the most inexcusable of sophisms, and has been commemorated by a name well known to logicians as the *petitio principii*.

Dr. Kennedy, moreover, in his indictment against hospitals, does not specify the cause of the mortality, and even if we were disposed to allow that his generalizations are correct, I think that we are warranted in concluding that the number of those who die from non-puerperal diseases, and from the accidents of labour, bears a very considerable proportion to the whole, and in giving statistics upon such an important question, this distinction should not have been admitted.

My friend, Dr. M^cClintock, in his elaborate and critical paper, has shown most forcibly the importance of this distinction, and in the tables which he exhibited, he has demonstrated in the most distinct and unequivocal manner, that the deaths occurring in lying-in hospitals from puerperal fever, and from other causes, are in fact very nearly equal.

He also has shown that in private practice—and upon this point the statistics are most reliable—that out of 95 deaths which occurred in the practice of Clarke, Cross, M^cClintock, Browne, Churchill, and Sir P. Dun's Maternity, there were :—

Accidents of labour,	-	-	37
Puerperal diseases,	-	-	31
Non-puerperal diseases,	-	-	27

Now, sir, this is a most important point, and in estimating the death-rate of any system cannot be overlooked.

I have alluded before to some statistics which have been published in the printed report of the discussion of the French Academy of Medicine. Although in Dr. Kennedy's paper, at page 295, he gives the general death-rate as 1 in 18½, yet he has omitted to give the particular death-rate as it occurred in each hospital during a succession of years.

I have copied the tables published in M. Depaul's communication. They embrace the returns for 5 years, and from 6 hospitals—the years are 1852, 1853, 1854, 1855, 1856.

In M. Depaul's communication before the faculty at Paris, he gives the mortality of the Maternité for 5 years, ending 1856, as 1 in 60.

In the Clinique d'Accouchement, for the same years, he

gives the proportion as	-	-	-	1 in	37
Hôtel Dieu,	-	-	-	1 in	38
Hôpital Saint Antoine,	-	-	-	1 in	40
Hôpital Saint Louis,	-	-	-	1 in	416
Hôpital Lariboisière,	-	-	-	1 in	24
Total,	-	-	-	1 in	48

22 *Observations on Puerperal Fever and Puerperal Mortality.*

Relative Proportion of Accouchements and Deaths from Puerperal Fever.

I.—MATERNITÉ OF PARIS.

	1852		1853		1854		1855		1856	
	Women Confined	Deaths	Women Confined	Deaths	Women Confined	Deaths	Women Confined	Deaths	Women Confined	Deaths
January, .	296	—	230	—	260	—	334	—	291	1
February,	289	4	299	8	302	—	265	1	271	1
March, .	311	5	339	19	236	—	235	—	351	3
April, .	263	—	314	18	303	—	257	—	317	20
May, .	239	1	225	7	305	—	259	1	51	30
June, .	205	3	165	1	239	—	206	—	55	—
July, .	236	1	193	1	262	—	241	4	184	5
August, .	196	1	208	7	239	1	255	5	205	2
September,	211	9	177	1	236	—	31	1	213	13
October, .	200	19	226	7	252	—	84	—	119	13
November,	217	3	216	—	289	—	102	—	185	7
December,	197	—	257	3	262	—	195	—	272	4
Total, .	2,860	46	2,849	72	3,185	1	2,464	12	2,478	99
Mean proportn.	1 in 62		1 in 39		1 in 3,185		1 in 205		1 in 25	

Total Accouchements, - - - 13,836
 Deaths known, - - - 230
 Proportion, - - - 1 in 60 about

Relative Proportion of Accouchements and Deaths from Puerperal Fever.

II.—CLINIQUE D'ACCOUCHEMENTS.

	1852		1853		1854		1855		1856	
	Women Confined	Deaths	Women Confined	Deaths	Women Confined	Deaths	Women Confined	Deaths	Women Confined	Deaths
January, .	98	—	123	7	104	2	116	—	108	5
February, .	103	—	52	5	95	1	89	1	81	2
March, .	117	2	2	—	97	3	116	4	79	1
April, .	92	1	—	—	93	1	104	4	107	—
May, .	117	3	76	1	81	—	99	2	66	7
June, .	76	—	96	2	54	6	88	2	9	5
July, .	96	1	74	2	76	—	91	1	—	—
August, .	104	1	93	4	58	1	85	1	65	—
September,	108	5	91	3	70	—	138	1	36	12
October, .	105	1	87	—	90	2	123	1	—	—
November,	108	—	87	8	89	4	116	3	38	—
December,	109	8	69	—	96	2	101	6	411	—
Total, .	1,233	22	847	32	1,003	22	1,266	26	630	32
Mean proportn.	1 in 56		1 in 26		1 in 45		1 in 48		1 in 19	

Total Confinements, - - - 4,979
 Deaths known, - - - 134
 Proportion, - - - 1 in 37

Relative Proportion of Confinements and Deaths from Puerperal Fever.

III.—HÔTEL DIEU.

	1852		1853		1854		1855		1856	
	Women Confined	Deaths	Women Confined	Deaths	Women Confined	Deaths	Women Confined	Deaths	Women Confined	Deaths
January, .	82	1	88	1	122	4	109	1	110	4
February,	93	4	93	9	145	5	112	2	107	6
March, .	80	1	95	7	145	3	105	—	113	6
April, .	92	1	104	9	159	3	96	3	109	4
May, .	98	2	108	4	131	1	83	2	154	10
June, .	59	4	87	1	107	1	86	—	188	11
July, .	84	—	101	—	109	1	94	—	136	5
August, .	73	1	104	—	81	—	85	1	105	—
September,	77	1	101	—	112	3	122	2	107	—
October, .	90	—	102	6	108	1	97	2	235	15
November,	85	1	120	1	108	2	109	2	156	10
December,	87	—	105	1	112	2	120	1	121	2
Total, .	1,000	16	1,208	39	1,439	26	1,218	16	1,641	73
Mean proportn.	1 in 68		1 in 30		1 in 55		1 in 76		1 in 22	

Total number confined, - - - 6,506
 Deaths known, - - - 170
 Proportion, - - - 1 in 38

Relative Proportion of Accouchements and Deaths from Puerperal Fever.

IV.—HÔPITAL SAINT ANTOINE.

	1852		1853		1854		1855		1856	
	Women Confined	Deaths	Women Confined	Deaths	Women Confined	Deaths	Women Confined	Deaths	Women Confined	Deaths
January, .	4	—	9	2	17	—	29	—	31	1
February, .	8	—	6	—	12	—	21	—	35	—
March, .	4	—	12	1	27	—	30	—	33	—
April, .	8	1	5	—	27	—	31	1	27	—
May, .	6	—	7	1	20	2	28	—	32	1
June, .	7	—	14	—	23	—	26	—	31	—
July, .	7	—	14	—	33	3	27	6	28	1
August, .	7	—	7	—	25	—	31	—	39	—
September,	11	1	12	—	24	1	30	1	25	—
October, .	8	1	8	—	27	—	31	—	39	—
November,	6	—	14	—	26	2	33	—	28	2
December,	10	1	7	1	27	—	27	—	35	—
Total, .	86	4	115	5	288	8	344	8	383	5
Mean proportn.	1 in 21		1 in 23		1 in 36		1 in 43		1 in 76	

Total Accouchements,	-	-	-	1,216
Deaths known,	-	-	-	30
Proportion,	-	-	-	1 in 40

Relative Proportion of Accouchements and Deaths from Puerperal Fever.

V.—HÔPITAL SAINT LOUIS.

	Women Confined	Deaths	Women Confined	Deaths	Women Confined	Deaths	Women Confined	Deaths	Women Confined	Deaths
January, .	52	—	60	—	78	—	65	1	64	—
February, .	46	—	51	—	69	—	67	2	64	—
March, .	57	1	60	1	73	—	71	—	83	—
April, .	48	—	71	—	71	—	68	—	75	—
May, .	47	—	55	1	71	—	62	—	73	—
June, .	68	—	38	—	65	—	63	—	63	—
July, .	51	—	59	—	69	—	55	—	65	—
August, .	55	—	63	—	64	—	58	—	73	—
September,	61	—	62	—	58	—	72	1	61	—
October, .	61	—	58	—	63	—	55	1	71	—
November,	51	—	57	—	63	—	62	—	58	—
December,	63	—	58	—	76	—	65	—	63	1
Total, .	660	1	692	2	820	—	763	5	813	1
Mean proportn.	1 in 660		1 in 346		0 in 820		1 in 152		1 in 813	

Total Confinements, - - - 3,748
 Deaths, - - - 9
 Proportion, - - - 1 in 416

Relative Proportion of Accouchements and Deaths from Puerperal Fever.

VI.—HÔPITAL LARIBOISIÈRE.

	1854		1855		1856	
	Women Confined	Deaths	Women Confined	Deaths	Women Confined	Deaths
January, .	—	—	47	2	59	1
February,	—	—	37	3	64	4
March, .	6	—	32	4	65	7
April, .	11	2	36	—	60	1
May, .	9	—	37	4	53	2
June, .	24	1	21	1	61	2
July, .	16	—	51	2	57	1
August, .	19	—	39	2	37	1
September,	20	2	53	3	51	—
October, .	25	2	48	1	55	4
November,	30	—	55	—	62	3
December,	31	1	49	—	61	0
Total,	192	8	505	22	685	26
Mean proportn.	1 in 24		1 in 22		1 in 26	

Total Accouchements,	-	-	-	1,382
Deaths known,	-	-	-	56
Proportion,	-	-	-	1 in 24

Now, sir, on inspecting these tables, it will be observed that the same apparent inconsistencies seem to have prevailed in those institutions as in those with which we are more familiar. Thus, during some months or seasons, there was a very small death-rate; during others a very considerable one. Again, the death-rate did not bear a constant ratio to the deliveries.

Thus, as an example, during those 5 years
there were confined in the Maternité, - 13,836 women.

The deaths were - - - 230 = 1 in 60.

In the 3 years there were confined in the
Lariboisière, - - - 1,382 women.

The deaths were - - - 56 = 1 in 24.

Again, in the Hôpital Saint Louis, during
the 5 years, there were confined, - - 3,748 women.

The deaths were - - - 9 = 1 in 416.

So much was M. Depaul impressed with this last fact, *i.e.*, the small comparative mortality in the last mentioned hospital, that he writes, "If it is difficult to understand why the mortality from puerperal fever is very trifling at the Hôpital Saint Louis, it is equally, or more so, when we take into account the different results that we observe at the Lariboisière; every body knows the excellent situation of this magnificent hospital, its chambers are large and well ventilated, the number of confinements is small, and still the proportion of cases of deaths from puerperal is very considerable."

The death-rate from all these 6 hospitals is 1 in 48.

In the year 1854, there were 3,185 women confined in the maternités of Paris—there was only 1 death.

In the year 1856, there were 2,478 women confined—99 deaths.

Contrast the 2 years' death-rate in the same hospital:—1854, 1 in 3,185, or .0314 per cent.; 1856, 1 in 25, or 4 per cent.

How are we to account for this? yet, it is similar to what has been observed in other institutions.

Take the Clinique d'Accouchement as another illustration:—

In the year 1852, 1,233 women were confined—22 died.

In the year, 1856, 630 women were confined—32 women died; or,

In 1852, 1 in 56, or 1.784 per cent.

In 1856, 1 in 19, or 5 per cent. died.

Again, in the Hôpital Saint Antoine, the entire number of confinements was, for 5 years, only 1,216, which gives an average of 243 per year.

In this hospital, in the year 1852, 86 confinements took place—there were 4 deaths.

In the year 1856, 383 confinements took place—there were 5 deaths; that is, 1852, 1 in 24; 1856, 1 in 76 died.

Thus we perceive that the number confined in 1856 was far in excess of the number confined in 1852, but the proportionate death-rate was inverse.

The tables of the Hôpital Saint Louis are well deserving of observation:—

	1852	1853	1854	1855	1856
Total Confinements, .	660	629	820	763	813
Deaths, . . .	1	2	0	5	1

Not a single death occurred in 1854, although the large number of 820 were confined in the hospital.

I think that those instances will be quite sufficient to quote in illustration of the fact of overcrowding not being generally contemporaneous with a high death-rate, and is exactly what we observe in our own institutions at home.

So much, sir, for the statistics derived from an inspection of the tables which I have copied from this printed report. I think that the information which they afford may be looked upon as most reliable, and there is moreover this peculiarity about them, viz., that an inspection of them will enable us to view, almost at a glance, the obstetrical work going for a period of 5 years, in the large city of Paris, and to examine the results of practice not only in the hospitals especially devoted to obstetrics, but also in those in which parturient women are assembled in institutions devoted as well to surgery and medicine; and we cannot fail to be struck with the fact, that in the large general hospitals, such as the Hôtel Dieu, the death-rate was not higher than in the special hospital, the Clinique d'Accouchement, it being in the former 1 in 38, in the latter 1 in 37, whilst in La Maternité it rose above that of the Rotunda Hospital, during the same period of years. The mortality in the Rotunda for those 5 years was 1 in 88; in the Maternité of Paris 1 in 60.

Having said so much upon the comparative mortality of those hospitals and our own, I will now sir, with your permission, and that of the Society, allude, in as concise a manner as is possible, to the opinions held by some of those experienced physicians as to the best means of arresting this disease, and limiting puerperal mortality. The question of treatment should not here be discussed, for I need scarcely say to you, sir, and those gentlemen assembled here, what I am sure is the experience of every one who has seen true puerperal fever or metria, that the question resolves itself into one of prevention, not treatment. It is so fatal that medicine has little or no control over it, and our treatment of it may be simply said to be this: to endeavour to keep the patient alive by wine, brandy,

ammonia, opium, iron, quinine as recommended by Beau, a medicine, however, which has not realized all that was expected and predicated of it, and of thus trying to prevent the tendency to death. In this respect, sir, however, I cannot conceive that it differs much from other blood diseases. Who has ever cured a case of typhus? Who has ever cured a case of small-pox? or, what medicines have the least effect upon real Asiatic cholera? I do not believe that we can, by medical treatment, cut short any of those diseases; they must go through their stages, and our highest art must be acknowledged to be, to guide, and to watch, to treat local symptoms which may be relieved, and to act in such a manner that nature may get time and opportunity to throw off the poison, or *materies morbi*. This, sir, I think all here will confess to be the only treatment possible, and we all must agree with Dr. Stokes, who stated on the last evening that the real disease is in most instances fatal. Indeed Dr. Stokes stated that he never saw a single instance in which recovery took place in private, although his practice extended over a period of forty years. Such, however, is not my experience, for I have seen some very bad cases recover, and I have no doubt that if we cease to bleed, leech, and give mercury, the proportion of deaths to recoveries will be in a decreasing series.

The question then, sir, on preventing the disease being acknowledged to be the all important one, and that which has raised the present discussion, let us hear what our French confreres say upon this subject.

Monsieur Trousseau, comparing the conditions of those women who are confined in our maternities with those in which country women are placed, says that amongst the latter they are confined, but they do not die (*on accouche et l'on ne meurt pas*), and this in spite of overcrowding, filth, and the dirt of their apartments in which they are confined; but he quotes the following facts to show that the disease does not seem to depend on overcrowding, viz.: at the *Maison de Sante d'Accouchements* in the years 1837, 1838, 1847, 1848, the number confined was 14,217; deaths, 312; mortality, 1 in 45. In the years 1852, 1854, 1856, the numbers confined were 11,482, the deaths were 638, or 1 in 17. He believes that contagious miasmata, producers of surgical typhus and puerperal typhus, are constantly lurking in surgical and obstetrical wards, and that at certain periods, and without a cause which we can define, they germinate in the system, and suddenly explode in either a sporadic or epidemic form.

M. Depaul says that, from what we know about puerperal fever, that it is nearly exclusively developed in houses or hospitals, in which are collected in large numbers women in their confinement; and that the cases observed in civil practice are in general only the emanation of the epidemics first concentrated in certain hospitals, that once the poison is developed, it is transmitted more surely and more fatally according as the number of

women assembled is considerable. His advice is to disseminate women as much as possible in different establishments, and, better still, to attend them at their houses. He says:—"I entertain the most profound conviction that this is the only means of causing the disappearance of, or notably diminishing those murderous epidemics, which come on periodically to carry grief into families, and to sadden the physicians who, having nothing to oppose to this disease but their uncertain medicines, only intervene to show their want of power."

M. De Chegoin says also that a number of women assembled together prove a focus of contagion.

M. Cruveilhier says, in the most forcible language:—"I state, with all the conviction of truth, that there is only one course to take in order to prevent the periodical return of those murderous epidemics—that is, the suppression of the great lying-in hospitals; the replacing them by attendance at the women's homes; in addition to which may be added a number of small hospitals, outside of Paris, capable of admitting twelve, fifteen, or twenty lying-in women, in which each woman may have a separate apartment.

M. Danyau says that lying-in hospitals are not only institutions for relief, but, moreover, they are educational establishments; and that so far from suppressing them, he would increase their number, the only way of doing away with large collections of lying-in women, and of thus rendering the invasions of puerperal fever more rare and epidemics less severe; at the same time thus multiplying the foci of instruction. He pays a high compliment to the Rotunda, which he had visited, and which he terms, "Le magnifique hôpital d'aconchement de Dublin."

He says, however, that everything he had seen forced the conviction upon him that small hospitals are preferable to large, and that even those can only be kept in a sanitary state on the condition of never being always full, of filling the wards by turns, and by adopting the best means of ventilation, deodorization, cleanliness, &c., &c.; and when signs of the epidemic make their appearance, by instantly closing the wards; and he finally recommends small maternities to be built as far away from *ordinary* hospitals as possible.

It is not surprising that puerperal should be so frequent in the maternité, for M. Danyau mentions a fact which is startling, viz., that in the year 1837 the pressure for admittance was so great that each salle or ward could be only evacuated twice during the year.

When this is compared with the system carried out in the Rotunda, I think the advantages will be found on the side of the latter, each of whose wards is thoroughly washed, cleaned, fumigated, ventilated, and whose bedding is renewed after each admission of patients.

M. Guérin also demands the suppression of maternities.

Now, what is the opinion of M. P. Dubois?

He says that the radical suppression of maternities could not be carried out in the actual condition of society; that there is no great centre of population which can do without them.

He also states that it is his belief that in the majority of cases the epidemic influence is in the first instance external, and that it only penetrates into the hospital after having manifested itself in the city; and he quotes the epidemic of 1836 as a striking example.

In this epidemic the first cases which he observed were outside the hospital, and the hospital was subsequently attacked. M. Dubois is of opinion that the suppression of maternities would make no difference in the actual state of matters, as regards the seat of the first epidemic manifestations, and would be a matter to be regretted, and would not be attended with equivalent results. He also quotes the instance of the fearful epidemic which attacked the village of Brackel, where, out of 28 women confined, 13 were attacked with puerperal and 12 died, and 2 only of the children of those women survived.

He also objects to their suppression, on the ground of their being educational establishments, and proposes to build, or annex to other hospitals without mixing them up together, buildings capable of receiving 600 or 800 women in their confinements annually.

To divide these buildings into two principal parts, equal in size and united by side galleries.

To divide each into chambers capable of holding ten beds, allowing an ample supply of air to each.

Now, in fact, he does nothing more than recommend a system which has been for a long time carried out in Dublin, except that less than eight beds are in each ward; he certainly refers to the arrangement of the Dublin Lying-in Hospital in very flattering terms.

M. Legroux says that when an epidemic shows itself in a maternity it nearly always makes its ravages elsewhere, and this even under the most favourable conditions.

He says:—Do we not see in an hospital on one day a whole series of women attacked, and those confined on the following day preserved? but the overcrowding exists as well for the one as for the other. Why, then, he asks, is the assembling of lying-in women more deadly on one occasion than another, when all the hygienic conditions are the same; and I think he hits off the answer when he says, it is that the *epidemic cause is independent of overcrowding*; but it strikes precisely there where its victims are in the greatest number.

We thus perceive that there is considerable difference of opinion amongst the Parisian physicians about the propriety of closing the lying-in hospitals, and that names of great celebrity are opposed to one another, as advocates of different systems.

What then are we to deduce from a consideration of all the facts

before us? I think that we may be fairly justified in concluding, from what has been elicited in the course of this discussion, that there is no doubt that there has been an increase of metria in the hospital during the last fifteen years, the period selected by Dr. Kennedy upon which to found his death-rate in this institution, and that the death-rate has been higher, during this period, than during any similar consecutive number of years.

That Dr. Kennedy has selected the most unhealthy years, upon which to strike his average death-rate for the hospital, but that he has omitted to strike an average death-rate from other most healthy groups of fifteen years.

That he has failed to prove that metria does not sometimes attack small lying-in hospitals, and even from his own tables it is evident that there is a considerable variation in the death-rates of small or cottage hospitals.

That circumstances would warrant us in believing, that except under conditions favourable to the development of it, there is not a very intimate connexion between the overcrowding of hospitals, and the occurrence of epidemics of metria, for we have seen that at times when hospitals have been overcrowded, the death-rate has been low, and that, on the contrary, when on many occasions the hospitals have not been overcrowded, the death-rate has been very high.

That the very group of fifteen years selected by Dr. Kennedy proves this. During this period the admissions to the hospital were far in diminution of those of previous years, and the deaths were far in excess.

That the high death-rate in several of the French hospitals does not seem to have been dependent upon an overcrowded condition, nor to bear any relation to the size, position, or sanitary conditions of the several hospitals, for we find that in one of the best situated, most modern, and best ventilated hospitals, La Lariboisière, the mortality during an epidemic was higher than in the Hôpital Saint Louis, which is an old hospital, and is situated in a poor and crowded part of the city, and cannot boast of all the improvements, in a sanitary point of view, in which the other excels.

We find that puerperal fever prevailed epidemically in all the Parisian hospitals in which accouchements take place, and was not confined to one or two; but attacked them all. This circumstance is strongly corroborative of the epidemic nature of metria, for otherwise it would most probably have selected some particular hospital.

That in the maternity attached to the two Dublin workhouses, the death-rate in one, on an average of twelve years, was only 1 in 213. Whilst in the other the death-rate was 1 in 79, although the conditions were in both cases very nearly similar; it must be remembered, however, that in the latter the death-rate was founded upon the statistics of only three years.

That in the Killarney Maternity the death-rate was high, viz.:—1 in

46, although there were no deaths from metria. This very instance shows the importance of distinguishing deaths which occur from metria, from those resulting from the complications and accidents of labour.

There cannot, too, be a more convincing proof of the existence of this disease independently of hospitals than the statement of Dr. Griffin, that it was prevalent in the neighbourhood and carried off many, whilst the hospital was totally free from it. This circumstance is exactly in accordance with what has been observed elsewhere, as in the remarkable epidemic which attacked the village of Brackel, in Westphalia, and which committed fearful ravages. Another instance was mentioned on the second evening of the discussion by Dr. Denham, who read an account of a very remarkable epidemic, which about twenty years ago traversed the course of the river Barrow, and was very fatal to those attacked.

That there is every reason to believe that the disease is for the most part epidemic in its nature, but that just as cholera is known to make its appearance most frequently where large numbers are assembled, although we are in ignorance, why such should occur occasionally and not always, so the metrial or puerperal poison appears to have an affinity for that condition of the air which is engendered, when large numbers of puerperæ are assembled, and is most frequently manifested under such circumstances, yet such is not always the case, and it is frequently seen to make its appearance under an opposite condition, or where there is not a very large number assembled, as in many of the instances referred to.

That although the same reliance is not to be placed upon statistics from external sources as upon those derived from the well-kept records of intern lying-in hospitals, or rather that we have not proper data as yet for statistics from such external sources, yet there can be scarcely a doubt that there must be a larger death-rate in large lying-in hospitals than there is in private practice, and in out-door attendance upon patients at their own homes.

So far, therefore, as our present knowledge extends, we are unable to account for all the inconsistencies which appear in studying this disease. We see on the one hand the disease prevalent in hospitals when they are not overcrowded, on the other hand we see it absent when the hospitals are so. We see it absent in unhealthy hospitals. We see it present in the most healthy. We see the most feeble persons escape. We see the strongest attacked, and succumb to it. We see it epidemic, on the one hand, and hospitals escape; on the other hand, we see it make a rapid invasion into an hospital and then as suddenly cease, whilst not a single case will be seen outside the hospital.

Such is my experience of this disease, and, from what I have read of it, such appears to be the experience of others. Whilst, therefore, agreeing with those who are of opinion that in an educational point of view large lying-in hospitals possess the greatest advantages, and are, moreover,

attended with the greatest benefit to the poor in many respects, yet I think that, on the whole, external attendance on an extensive scale, combined with an internal accommodation limited in extent, will be found to be the best, and one the most free from a very large death-rate. This is the system I think to which we are drifting with the tide, and the success of the experiment tried at Sir P. Dun's, is one of the strongest evidences in my mind of this tendency. I think that a fair way of putting the question would be this, viz. :—would any of the gentlemen, experienced in lying-in hospital management, if they had the power, open a large lying-in hospital with we will say 100 beds. I think—I may perhaps be in error—that with their present experience, they would not select to have so great a number of beds, and that they would commence, and be most likely to continue, with a smaller number.

I must, however, decline to accept Dr. Kennedy's solution or explanation of the modes or propagation of metria as final, and I do not agree with him in thinking that the isolation of patients in huts in the garden of the lying-in hospital, and the closing of the noble wards of this grand institution, will put a termination to this scourge of lying-in women; on the contrary I think that, by attention to ventilation and other matters of the kind, and by watching carefully over all the minutiae, which is so necessary and important a matter—but, above all by diminishing the number of admissions, and giving a larger out-door attendance, which I am sure the present master, my friend, Dr. G. Johnston, aided by his skilful assistants, will do, that, at all events, the death-rate will not increase, and I cannot anticipate that it will be a larger death-rate than it would be in huts if the plans recommended by Dr. Kennedy be carried out—plans founded upon hypotheses, and based upon data not sufficiently proven. With regard to the hut system recommended by Dr. Kennedy, I think the case mentioned on the last evening by Dr. Stokes, quite settles that question. For he adduced an instance of the fact of its being a constant resident at Ballarat, in Australia, amongst the settlers who lived in nothing but huts. So that I think we may say little more about this; but I may take this opportunity of expressing the feeling of admiration—a feeling shared, I opine, by every gentleman who had the good fortune to be present on that occasion—of the lucid, philosophical, and logical observations which were made upon that evening in reference to this important subject by our illustrious visitor, Dr. William Stokes.

POSTSCRIPT.

Dr. E. Kennedy gives the death-rate of the Waterford Hospital as 1 in 295. This is only for a limited period—viz., 23 years, 1844 to 1868. The gross mortality for the entire period—viz., 1838 to 1868—is 1 in 227, the number of births being 3,409, and the deaths 15.

